

## TORNADO OF APRIL 14 NEAR PENSACOLA, FLA.

By WM. F. REED, JR., Observer, Weather Bureau.

A tornado was reported near Beulah Settlement, twelve miles northwest of Pensacola, Fla., at 10 p. m., April 14, 1905, eastern time.

The barometer readings over the eastern and southern portions of the country on the morning of the 14th were unusually low. Three areas of low pressure were charted, which were encircled by isobars of 29.7 inches, one around Denver, Colo.; one around Palestine, Tex., and the other inclosing Wytheville, Va., and Charlotte, N. C., with indications of southeasterly movement of high pressure from the northwest. The morning barometer at Pensacola was 29.81 inches, with the relative humidity 93 per cent. A local thunderstorm occurred from 6:05 to 7:10 p. m., and came from the southwest. The highest velocity for the day was twenty miles, from the southwest, at 11:48 p. m. The sky remained cloudy during the night, with strato-cumulus clouds prevailing.

The following account of the tornado was obtained from Mr. Arthur Spare, Cantonment, Fla.:

## NOTES AND EXTRACTS.

## UNUSUAL WEATHER AT DODGE, KANS.

The following is a letter from Mr. E. D. Emigh, assistant observer, temporarily in charge of the Weather Bureau station at Baltimore, Md.:

TO THE EDITOR:

Permit me to add the following note to your interesting comment on my article concerning the unusual weather conditions at Dodge, Kans., during the last week in February and the first week in March, published in the MONTHLY WEATHER REVIEW, p. 51, for February, 1905.

A short explanation of certain features of the phenomenon may not be amiss. Upon striking the ground or cold objects against which it was driven by wind, the mist formed into a solid sheet of ice, as assumed in your remarks; but when it came into contact with woolen clothing the liquid form was retained, however long the clothing had been exposed to the cold air. I presume, therefore, that it is correct to assume that the temperature of the wool must have been sufficiently high to overcome the subcooled condition of the mist.

With reference to the last paragraph of your comment, it was, of course, necessary to melt the ice collected in the receiver of the rain gage before the amount of precipitation could be measured.

## SNOW AND FROST CRYSTALS.

Referring to the preceding letter from Mr. E. D. Emigh, we reprint the following extract from the annual report of the Chief Signal Officer for 1891—being a part of the report of Prof. C. F. Marvin, on "Maximum pressure of aqueous vapor at low pressure," pp. 351-386:

## NOTE UPON THE ABNORMAL FREEZING OF WATER AND CORRESPONDING VAPOR PRESSURE.

During the progress of the vapor-pressure work considerable difficulty was experienced in freezing the water capsules used in the experiments, and the particular results obtained indicate the possibility of water retaining its liquid condition under very unusual circumstances.

Mention was made on page 360 of the method of breaking the water capsule by freezing. For this purpose the tube *a*, fig. 9,<sup>1</sup> was wholly surrounded by a freezing mixture of salt and ice. In many instances, even after one or two hours' exposure in this manner to a temperature continuously from 3° to 5° below zero, F., the water in the capsule remaining unfrozen. It is true the transfer of heat from the water through the vacuum must undoubtedly have been very slow, taking place quite wholly by radiation, yet the temperature was certainly very low, and the phenomenon of not freezing a real one, as the same result was obtained with a precisely similar capsule directly immersed and moved freely about within the liquid freezing mixture. In this case there could be no doubt as to the temperature. In both of these cases, although it was possible to considerably agitate and jar the capsule, yet the water so very nearly filled it as to be but very little disturbed; nevertheless in the case of the free capsule the small bubble of space within could be made to move about from end to end, etc., yet without the slightest effect to induce solidification. It was therefore found necessary to lower the temperature still further to effect freezing, which was generally suc-

The storm of the 14th of April was undoubtedly a small tornado, or, as we called it, a "twister." It seemed to be the culmination of two storms, starting close together. It rained very hard for a few minutes, when it began to hail from the northwest. The hail came twice from that direction in intervals of about five minutes, the hailstones both times being round or slightly oblong, smooth, hard, and as large as hazel nuts. A second hailstorm came from the southeast, with heavy rain and wind. The hailstones in this instance were flat, about three-fourths of an inch in diameter and three-eighths of an inch thick, with rough, ragged edges. It was one continuous storm of wind, rain, and hail for the time it lasted, and did not slack up raining when the wind changed. The hardest part of the storm lasted for 30 minutes, the rain continuing an hour longer. There was plenty of lightning, but not much thunder. The amount of rainfall can not be determined. A funnel-shaped cloud was described to me by two neighbors who saw it. There was a roaring sound accompanying the storm, which came from the southwest and moved generally toward the northeast, showing zig zag path in some places. The presence of a whirl is plainly shown by the distribution of debris. At one place the path is 50 yards wide, the trees on one side lying with their tops toward the southwest, and fifteen steps from there is a tree twisted off with its top the other way. I traced the path for about one-half of a mile. It mixed up things badly at my old house, where the path is fifteen yards wide, and will take much of my time to straighten up matters again.

cessfully accomplished at temperatures from -10° to -15° F. I am disposed to believe, however, that the real temperature of the water in such cases may doubtless have been little lower than -5°, but that it could be appreciably higher than 0° seems scarcely credible under the circumstances.

In more than one instance solidification took place within the capsule, but peculiarly enough it was not broken thereby, and, in consequence, I have even been to the annoyance of entirely refilling the apparatus in order to introduce a new capsule of thinner glass and presumably less strong. Subsequent experience, however, led me to believe that in all these cases the failure to break the capsule was really due to the fact that a part only of the water was frozen, and had sufficient time been given, the capsule must surely have burst. It was at first imagined, since the solidification was practically instantaneous, that the whole mass froze at once. This, however, does not appear to be the case, as is indicated by the following considerations: Water in freezing must give off about 140 units of heat. If now, without freezing, the temperature be lowered to, say, -5° F., that is, 37° below the normal freezing point, about 37 units of heat have been withdrawn in lowering the temperature more than is really necessary. When, therefore, solidification once starts the dissipation of 37 units of the latent heat of freezing can take place with great suddenness and operates to warm up the whole mass of water to its normal freezing point, and further solidification can take place only on the slow dissipation of the latent heat.

Phenomena of this character were repeatedly observed with different capsules, and subsequently a few other experiments in the same direction were made. Thus, a capsule of somewhat larger dimensions was attached to a piece of spirit-thermometer tubing having a comparatively fine bore. This was filled with well-boiled, distilled water and sealed up after the manner of a thermometer.

The elimination of air from the water or the space above was by no means so perfect in this thermometer as in the capsules used in the vapor-pressure tubes. Marks were made on the tubes at the points opposite the top of the water column when the bulb was in ice, and also at the temperature of maximum density. Thus, the water was made to roughly indicate its own temperature, but more particularly showed the changes in volume with temperature. When the bulb was immersed and moved about within the freezing mixture, the column would soon fall to the point of maximum density, and would gradually ascend again and pass considerably beyond the line marking the volume at the freezing point, showing thereby that the *expansion observed to take place in water, from the point of maximum density to the normal freezing point, is continuous when under any circumstances the water may be cooled below this normal freezing point without solidification.* As soon, however, as the water reaches the point at which it will start to freeze, there is a very sudden solidification of a part of the water, and the increase in volume is very great, forcing the unfrozen water far up into the chamber at the top of the stem.

The structure of the ice in these cases, as, in fact, in all others of sudden freezing, is coarsely crystalline, presenting many arrangements of long, interlacing needles, and giving a somewhat milky color to the whole. An instant's exposure of the frozen bulb to the air quickly loosens the ice from the walls of the bulb, and as it melts slowly can be seen to rise to the top side of the bulb as the latter is revolved or turned about into different positions. The ice seems to be a comparatively compact mass throughout, not shell-like, as might be imagined.

When the small quantities of water used in the vapor-pressure bulbs were subjected to low temperatures, here also freezing never took place at the normal temperature. As the temperature of the bath in which

<sup>1</sup> Not reproduced.

the bulb of the pressure apparatus was immersed was gradually lowered during the observations, it was customary to frequently lift up the bulb and examine if the water was frozen. This was never found to be the case until the temperature was many degrees below the normal freezing point, and no amount of agitation and slopping about of the water had any visible effect in hastening the freezing. It was not practicable to actually witness the freezing; after passing some low temperature the water was found to be frozen. On several occasions the mercurial columns were continuously watched during the lowering of temperature. Presently a sudden increase was observed to take place in the vapor pressure, followed by nearly as sudden a return to the previous condition. The mercurial columns in these cases never reached closely their positions corresponding to the pressure at the normal freezing point. This action takes place no doubt at the instant of freezing and indicates a rise in the temperature. The large surface of exposure of the bulb prevents any great difference of temperature between the vapor within and the bath, and the small quantity of water is very quickly entirely frozen.

On page 368 it is shown how the water may be made to frost itself over the inner surface of the large bulb. On one occasion this was tried by placing the large bulb in the bath at a temperature of about 25° F. After some time the water had entirely distilled into the large bulb, and this being as much as 7° below the freezing point it was expected to find an even coat of frost inside. On examination, however, the vapor had not condensed as frost, and water in the liquid state only was found. The temperature was then lowered, as usual, to nearly 5° before the water, as shown by frequent examinations, and notwithstanding much agitation, was found frozen.

The water in this same bulb, on other occasions, was always frozen at a temperature of 10° as was also the water in the other pieces of apparatus. As near as could be told the temperature of freezing was about 11° to 14° F. We have in this then not only the abnormal existence of water in the liquid state at very unusual temperatures, but a somewhat corresponding behavior of the vapor which does not necessarily solidify on condensation at a temperature lower than the normal freezing point.

This experiment of distillation was repeated on another occasion with the temperature of condensation at about 5° F. In this case the water in the small bulb was almost instantly frozen by its own evaporation and persistently remained so despite moderate applications of heat, such as the hand, warm water, etc. After all the ice had distilled into the large bulb the latter was found to be beautifully coated inside with thin ice crystals.

In order to examine some points respecting the freezing of water under these circumstances, a bulb not encumbered with the manometric tubes and mercurial columns was exhausted and a capsule of water broken within. In this case the water capsule froze and burst after about two hours' exposure in an ice and salt mixture at a temperature not at any time higher than -3° F. The water in this bulb behaved in all respects similar to that in the regular vapor pressure tubes and even when violently shaken could not be frozen until cooled to a temperature of about 12° F. If, however, the water, once frozen, was almost entirely melted, leaving only a small fragment of ice, and then again cooled slightly below the normal freezing point, the whole mass soon became solid. The presence of the small fragment of ice at starting seems necessary to induce solidification at the normal freezing temperature.

When air was admitted to this bulb and the water shaken about for a few minutes, it could be frozen at 25° F.

#### ABNORMAL VAPOR PRESSURES.

In all the observations of the vapor pressure, when the temperature of the water was below the normal freezing point and the water still in the liquid state, the pressure was always observed to be greater than when the water at the same temperature was in the form of ice. The following table shows the differences found in the pressures under these circumstances:

| Number of observations. | Temperature. | Pressure. |             | Difference. |            |
|-------------------------|--------------|-----------|-------------|-------------|------------|
|                         |              | From ice. | From water. | Ice—water.  | Ice—Broch. |
|                         | ° F.         |           |             |             |            |
| .....                   | 32.00        | ?         | 4.568       | ?           | ?          |
| 3.....                  | 24.83        | 3.280     | 3.419       | -0.139      | +0.007     |
| 4.....                  | 19.87        | 2.591     | 2.771       | -0.180      | +0.001     |
| 4.....                  | 14.84        | 2.042     | 2.237       | -0.195      | +0.005     |
| 1.....                  | 10.06        | 1.608     | 1.786       | -0.178      | -0.022     |

The same methods have been followed in combining the results by the different tubes, as have been already described in connection with Table III.

Although, in all cases, the water would remain liquid at temperatures far below the normal freezing point, yet, after being once frozen, no melting could be detected until a temperature of 32° was reached. At this point melting always began, and the vapor pressures thus observed can not, therefore, be considered strictly as observed over ice, as it was impossible to prevent incipient melting, giving rise to at least a thin film of water over the ice. The vapor pressures measured over ice at tem-

peratures a fraction of a degree below 32°, and given in the second value in Table III, agree so closely with those over water *exactly* at 32° as to lead to the conclusion that the vapor pressure over dry ice at the melting point is exactly the same as that over water at the normal freezing point.

The striking agreement with Broch's tables of vapor pressures from water at temperatures at which it should be frozen, but still remains liquid, is remarkable and seems very significant.

The last value in the table above is the result of only one observation taken with tube No. 8, which may account, in part at least, for the larger difference in the last column.

The peculiar behavior of water as thus discussed, has in some particulars been observed by others, though the general impression appears to be that a little agitation or disturbance is sufficient to induce freezing whenever the temperature is below the normal freezing point. This is not at all the case with the experiments above described, as the temperature at which solidification took place seemed quite definite and depended wholly upon temperature.

I have not been able to find any note of the abnormal freezing of water under quite the circumstances of my own experiments.

Boussingault (Comptes Rend., Tome LXXIII, p. 77) perfectly filled a very strong steel cylinder with water at its maximum density which was afterwards not frozen upon long exposure at a temperature of -24 C. Here the effect of enormous pressure comes into play.

Dufour (Ann. Chim. et Phys., T. XLVIII, p. 370) discusses the non-freezing of globules of water in suspension within another liquid of the same density.

Water in very fine capillary tubes has also been observed by Sorby (Phil. Mag., 4th series, Vol. XVIII, p. 105) to retain the liquid state at very low temperatures. Fine mist particles have also been observed to be in the liquid state at temperatures much below the freezing point.

The almost perfect elimination of air and dust particles from the water seems to have very much to do with these peculiar phenomena of solidification.

It is evident that careful search will reveal many forms of crystals growing in cold weather in unlooked for places. In fact in cold countries and in the coldest portions of refrigerating establishments one ought to be able to reproduce at will many hitherto unknown forms of ice crystals. It occurs to us that the nuclei about which crystallization begins or the chemical and physical peculiarities of the solids on which the crystals form, or the quantity and kind of gases dissolved in the pure water may have much to do with their shapes and internal characteristics. With regard to hollow crystals, tubes, or columns of ice, and the hollow bubbles within lamellar crystals and the formation of ice columns in gravelly soil or sheets of ice columns protruding from crevices in the bark of a tree, we would refer to the American Meteorological Journal of April, 1893, Vol. IX, pp. 523-525, where the present writer has given the following statement of the interaction of water and ice as the crystals and columns grow by accretion.

#### ICE COLUMNS IN GRAVELLY SOIL.

By PROF. CLEVELAND ABBE.

A very pretty subject for observational elucidation, during the spring and fall, consists in studying the true method of formation of the little slender columns of ice that are found at the surface of gravelly soil in moist places after a clear, cool night. The best account hitherto given of this phenomenon is that by Dr. John Leconte in the Proc. Amer. Assoc. Adv. Sci., 1850, Vol. III, pp. 20-34.

My own attention was first called to the subject at Cambridge, Mass., in the spring of 1863, and since then this formation has been observed by me almost every year. It is undoubtedly not only very common in our latitudes and soils, but is also quite an important item in agricultural soil physics.

After a clear, cold night (whether windy or not seems to be immaterial) the surface layer of a moist gravel bed is usually found to be raised up an inch or two by a mass of vertical columns or needles of ice. The lower ends of these do not penetrate the soil below, but rest upon what had the previous evening been the next lower layer of gravel. If the wind be very strong the columns are best developed in the sheltered spots. As soon as the sun strikes the raised layer of gravel and warms the tops of the grains they sink a little and then the solar rays perforate slanting holes into the mass of ice crystals. Only once have I seen the corresponding phenomenon of a thin sheet of parallel ice columns exuding from a vertical crevice in the bark of a tree, many beautiful examples of which are given by Professor Leconte and Sir John Herschel.

Although dissatisfied with the explanation of this phenomenon, as given by Leconte and others, I was, however, unable to frame a more plausible hypothesis until lately, when considering the studies made by

Mr. Milton Whitney on so-called capillary water, it has seemed to me that the following explanation is plausible:

During clear, spring nights the ground a few inches below the surface retains the warmth of the preceding sunshiny day; by reason of this any moisture that may be there present is preserved as liquid water and works its way upward to the atmosphere either as vapor between the gravel grains, or as a thin film of water inclosing each grain and traveling from one to the next up to the very surface itself by capillary action. The films which would inclose the grains of the uppermost layer of gravel are apt to be quickly evaporated, but the films inclosing the grains immediately below that layer are protected from the wind and from diffusion into the dry air above. No sooner have the upper surfaces of the uppermost grains cooled by radiation below the temperature of their lower sides than there begins a process of conduction of heat upward through the body of each grain of gravel. Very soon moisture condenses as a liquid film on the cooling lower side of each grain, and soon afterwards on the upper side of the grain of gravel immediately below it, and so on gradually, as night advances, for a considerable distance downward. When now the uppermost grain has cooled below the freezing point then the next thing that happens is that on its lower surface its thin film of water freezes, and this implies that the water shall freeze last at those points where the upper grain comes in contact with the next lower grains because those points receive a little heat by conduction from below. Thus, at these points the frozen films protrude downward, and the projecting knobs may be considered as minute circles of ice formed in the watery films inclosing the lower grains while the rest of each such film still remains liquid.

Now, liquid water has a great surface tension, while ice has none, and the watery film inclosing any lower grain will almost instantaneously press in under and lift up the little speck of ice that has formed at the point of contact. The loss of heat by radiation from the upper grain continues steadily, and a steady process of conduction of heat goes on from the water of the lower film through the ice crystal at the point of contact up to the upper grain. Hence, there is a correspondingly steady formation of ice at the points of contact, and a continued renewal of the lifting process takes place until, in the morning, we find the upper grains, and even large pebbles, raised up several inches on the tops of tall columns of ice.

Will not some one devise a miniature repetition of this process in the physical laboratory? Let a small vessel be supported by three rounded metallic knobs resting on surfaces which are covered with thin films of water. The vessel should not be so heavy as to force out the films of water, and the surfaces of contact should not be so large that the circular areas have too large radii. The constricting power of a circular hole in a thin film increases inversely as the radius of the hole. A freezing mixture within the vessel should by conduction send down enough cold to produce ice at the points of support, and yet not enough to rapidly freeze any large portion of the film of water. The success of the experiment and of the natural formation of tall columns of ice must depend upon the radii of the frozen circular films at the points of contact and on the rate at which the heat that is absorbed from the lower film, is conducted through the ice and lost by radiation. The delicate adjustment of conditions that will bring this about makes this formation a very interesting physical problem.

When the outer air is frosty, while the sap is pressing up the body of the tree, a thin film of moisture may possibly be supplied from within as fast as the outer film at the surface of a crack may be frozen and lifted, and may thus form the exudations from the trees described by Doctor Leconte and also observed by myself and others.

This explanation of the growth of hollow columns of ice in gravelly soil, applies with slight changes to the hollow stems and plates of snow crystals. The whole subject of the growth of crystalline forms needs elucidation.

The very interesting investigations of Mr. Bentley on snow and frost crystals remind us of the following article copied from the Report of the British Association for 1858, part 2, pp. 40-41:

ON A FRESH FORM OF CRYSTALLIZATION WHICH TAKES PLACE IN THE PARTICLES OF FALLEN SNOW UNDER INTENSE COLD. BY J. WOLLEY, M. A.

In passing a winter in Lapland, it is impossible, whether in observing the tracks of animals, or merely considering day by day the condition of the roads for sledging, or of the snow for the use of snow-skates, not to be struck by the very variable character of the snow, partly caused by winds and fresh falls, partly by the condition of the rime or hoar frost upon the surface, but mainly, as it is soon found, by an alteration in the character of the mass of fallen snow.

In Lapland, as elsewhere, the snow as it falls is of several kinds. But whatever its character, it at first lies more or less lightly on the ground and if the weather is still and not very cold, it may so remain for days; but if the cold increases, the snow rapidly sinks; it becomes at first like sand, is crisp to the tread, bears the smaller animals, and soon becomes

suitable for the skidor or snow-skates. When the cold has continued, probably many degrees below zero of Fahrenheit, for two or three weeks, not necessarily consecutive (the phenomena are more especially to be observed in the cold months of January and February), the snow beneath the surface is found to be made up of large pieces of a quarter or a third of an inch in diameter, glittering in the sunshine, clear, heavy, highly moveable upon one another, and seen upon even a hasty examination to be of a beautiful crystalline structure. On a closer inspection, they are found to be somewhat irregular in shape, with the outline of more or less complete hexagons with sides of unequal lengths; they are formed around nuclei by no means placed centrally, often quite where one side of the hexagon should be; and they grow in layers of bars one outside the other, often larger in section, as well as longer, as they recede from the nucleus; these bars (learned gentlemen will excuse me for not describing a crystal more properly) are free from one another, except at the angles; those of each layer lie in one plane, often not the same as the layer which preceded them lies in. At the angles are usually small crystalline projections, rising apparently perpendicular to the general surface of the crystal. These crystals are broken with a slight force; and many of those where the snow has been crushed have lost their nuclear portion, but retain the true hexagonal form.

Snow, in the condition of which I hope to have given at least some faint notion, is called *hieta lumi*, or "sand-snow", in the Finnish language. It yields more water, and hence, even when it is covered by more recent snow, the Laps take the trouble of digging down to it to fill their kettles with. These primitive people also use it in its dry state for washing or cleansing their clothes. After first exposing to the external cold for some hours the dresses they wish to purify, for reasons which I need not further explain, they beat them with sticks upon and under a heap of sand-snow.

When the winter covering of the ground is in this sandy condition (perhaps the moveable state of such shell-sand as that of John o'Groat's house may best represent it in one respect, and the appearance of a bag of clean crystals of salt give some idea of it in another), it is a great advantage to all the animals of the country in supporting their weight, and is a special comfort to the reindeer, from the facility with which they can remove it with their forefeet so as to get at their food beneath. Though intensely cold to a naked hand, it is much better than fresh snow for lying upon, as it does not yield too much to the weight of the body, and does not get into the necks of the clothes, or melt in the fur. I may mention, that with only a thick pair of stockings on, one can walk for some little distance from a bivouac without risk of getting either wet or cold in the feet; and before a fire in the woods this snow never becomes sloppy, but seems to disappear only by evaporation, which greatly adds to the facilities of passing the long winter nights in a Lapland forest. The same thing is in a great measure true in the spring; the snow is very rarely to be found in that miserable state which marks the breaking up of a snow in England.

Concerning the formation of these crystals, I made experiments by burying in the snow at certain intervals of time, chip boxes, some empty and some containing fresh snow; I was prevented from fully carrying out and registering my observations, but I found that the changes went on in the boxes equally with the external snow, and in the boxes that contained nothing but air, but, nevertheless, were not so tightly closed as to prevent the transmission of air containing water in solution, crystals attaching themselves to the sides and top, but never to the floor, of the box, which crystals greatly resemble those in the snow; they were, however, often much longer, even to upwards of half an inch in length. In the course of my observations, I found that this sand-snow formed principally in open places, on lakes, bare soils, etc., growing less on spongy grounds, scarcely at all upon logs of wood or outbuildings.

#### TIDAL PHENOMENA.

Associated with the tides are many erroneous notions which appear from time to time in a variety of forms. Several of these constitute the essentials of an article by Mr. S. R. Elson, entitled "Tidal phenomena", which appeared in The Journal, Calcutta, India, for April 9, 1905. It is only because of the persistence of these and similar errors that notice is here taken of the article.

One of these is that the time required for the force of gravitation to traverse space may be considerable, and thus aid in explaining the fact that spring and neap tides do not occur simultaneously with the syzygies and quadratures. This idea is at least as old as Daniel Bernoulli's essay on tides; but that philosopher hardly deemed it worthy of serious consideration.

Another error consists in assuming that credence is still given by tidal authorities to the notion that a tide wave travels around the earth from east to west following the moon. While Mr. Elson justly condemns this notion, his explanation amounts